

November 11, 1907, quoted by Dr. Chree, stood alone, one would readily accept his explanation, but since that date many similar differences, though not quite so large, have been recorded. In fact, the noticeable point about the so-called isothermal layer is the very large differences of temperature that are found at the same time over places a few hundred miles apart, and over the same place within a period of twenty-four hours. Because we cannot explain the phenomena, are we, therefore, to doubt their existence? It is perfectly natural to do so; the question is simply one of the credibility of the evidence.

The evidence is of various kinds. If one of the instruments used in England be completely immersed in a bath of liquid by an observer A, the temperature of the bath being, say, between $+30^{\circ}\text{C.}$ and -50°C. , a second observer B having the record and the instrument can ascertain within 1°C. , or at the most 2°C. , the temperature of the bath used by A. Why, then, cannot B equally well ascertain the temperature of the air through which the balloon has carried the instrument? Secondly, these instruments are carried up by a balloon travelling through air that has been in contact with the balloon; the balloon in general bursts, and they fall, moving now at a much greater speed, since in England we use no parachute. Two traces are made, the one showing the temperature during the ascent, the other during the descent, but it is not often possible to say which is which. As a rule, the two traces are quite distinct; mostly, one indicates a temperature of from 1°C. to 3°C. below the other throughout, but sometimes the traces cross and re-cross each other. However, the point is that the two traces are practically identical; any peculiarity of gradient shown on the one is reproduced at the same height on the other. Now I think it lies with those who imply that our instrumental records are untrustworthy to explain this. If the temperatures shown by these two traces are not the approximate temperatures of the air, what are they? Systematic errors could not be the same in the different circumstances of the ascent and descent. It is inconceivable that casual errors could always so combine as to give errors of the same magnitude in pairs time after time. It is even less likely than that a man, drawing coloured balls from a bag, should draw the same colour in every two consecutive draws, for not only is the general trace reproduced, but every peculiarity in it is also reproduced.

Thirdly, the results obtained on the Continent and in America agree perfectly with those obtained with different instruments and a different system in England. This alone is not a good argument against the possibility of large casual errors, since casual errors are eliminated in the means, but the two sets of observations are as yet not very numerous—about 100 in England—and they show the same general relation between the temperature and height of the isothermal column and the height of the barometer at the surface.

Dr. Chree, from the last paragraph of his letter, appears to think that the instrument makers supply the scale. This is not the case in England, and I do not think it is abroad. Almost every instrument sent up in England to the present time has been made here. The University of Manchester is responsible for the scales of those that it sends up, and I am responsible for the scales of the rest. These scales are verified before and after each ascent. The lag in our instruments is very small, since we depend on the expansion and contraction of a strip of very thin German silver, but I do not see that the lag affects the general question, since it will be largely eliminated if we take the mean of the ascent and descent.

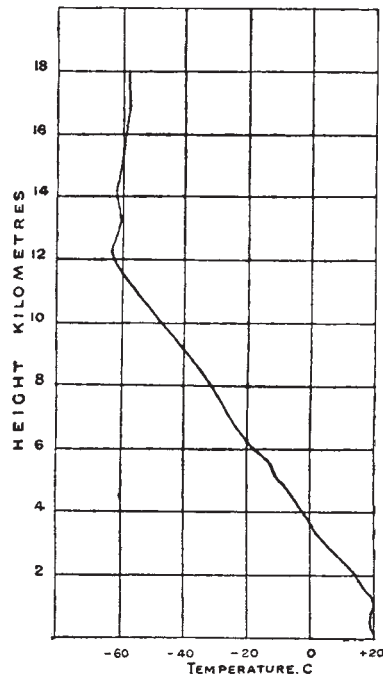
W. H. DINES.

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As one who subscribed to the "Confession of Monaco," may I be allowed to say that no definitions of the names stratosphere and isothermal layer were supplied at the conference as those present understood the terms? The meaning of a word has often divided the orthodox from the heterodox, and for the benefit of Dr. Chree, and also of "heretics in England," I will endeavour to make the matter clearer. Balloon ascents show that, apart from irregularities near the surface, the temperature of the air

decreases with height fairly regularly up to a certain point; above this point the regular decrease ceases, and for still greater heights the temperature changes are very small; sometimes there is a small increase, sometimes a small decrease, and sometimes the temperature remains almost constant up to the greatest height reached by the balloon. At any one place and time it thus appears that the atmosphere is divided into two layers, which differ markedly from one another in their vertical temperature distributions.

A diagram from an actual ascent made here on October 1 of last year shows the two characteristic temperature gradients. To the upper layer the names isothermal layer and stratosphere have been given; the latter name is due to M. Teisserenc de Bort, who surmises that the lower layer, or troposphere, is the part of the atmosphere concerned in the vertical circulation associated with cyclones and anticyclones, while the stratosphere lies above such movements. The name isothermal layer is not a fortunate one; certainly none of the orthodox who were assembled at Monaco would maintain that the upper layer is isothermal either in time or in a horizontal direction. Some less misleading term might have increased the number of the "elect." Both terms, however, are now in general use, and give definite names to a definite thing, which, as Huxley said, is the object of nomenclature.



The characteristic temperature gradient of the upper layer has been found over all parts of Europe, over the Atlantic, and over North America, but near the equator, if it exists at all, it is at a much higher altitude than in temperate latitudes. Its absence over the equator, and the fact that lower temperatures have been recorded there than in any other part of the atmosphere, seems to me to be a further proof, if such were needed, that the temperature gradient of the upper air recorded in other places is not the result of instrumental error.

CHARLES J. P. CAVE.

Ditcham Park, Petersfield, June 6.

The Sense of Proximity.

IN NATURE for March 11 there is an interesting account by Dr. McKendrick of some investigations by Kunz, of Mülhausen, and Prof. Griesbach, on the senses of the blind. Among other points that he refers to and discusses is the question of the ability of the blind to avoid obstacles and find their way about. This calls to my mind some observations and experiments which I made upon myself some eleven years ago with reference to my ability to find my way about with my eyes shut or in the dark. These I had intended to extend and amplify, but up to the present these further experiments have been crowded out by press of other work.

Many people have the feeling that if, for instance, they are in a room in the dark, they have some perception of their relation to objects in the room, and particularly can appreciate when they are near one of the walls. I can remember having had this feeling for many years, but never had the opportunity of putting it to scientific test

until the date I mention, when I was resident medical officer to a large London hospital.

Working as I was frequently until a late hour in the pathological laboratory, which opened off the entrance hall, I had, in order to reach my room, to cross the hall obliquely and enter the corridor by a wide door, some 6 feet wide, with folding glass doors, which were, as a rule, fastened back. The hall and corridor were unlighted. I usually walked well into the hall from the door of the pathological laboratory, turned to the right when I thought I was opposite the door opening into the corridor, and then walked straight forward between the doors. I found, a good deal to my surprise, that though in the dark (even though I shut my eyes) I could judge as I walked through, very accurately, to which of the two doors I was nearest. I made a large number of observations, and the constant result was sufficient, I think, to preclude any idea of mere coincidence. I found I could even form a trustworthy estimate if I was only a few inches nearer one side than the other; and, further, if I gradually moved towards one or other side, when I got within a few inches of the door I "felt" that I was getting very close to it. The way in which I felt this is difficult to describe, but the sensation of "nearness" was situated in my face, on my forehead and cheeks, and seemed to be particularly keen on turning my cheek in the direction of the surface that I was approaching. The conclusion that I came to was that there were two different processes involved; in the first case (1) the nearness of a solid body was made evident by difference in the reflection and resonance of my footsteps as I walked, and in (2) the differences in the reflection of the heat of the face from a surface at varying distances were the cause of the sense of nearness or farness. It will be seen that I had arrived at almost precisely the explanation which Dr. McKendrick puts forward as the explanation of the power of the blind to recognise their relation to externals.

(1) To test my theory of sound reflection I tried the effect of walking in stockinged feet, and found that it sensibly diminished my power of recognising my position; this is, of course, quite analogous to the difficulty, which Dr. McKendrick describes, experienced by the blind when there is snow upon the ground. A still more conclusive test of the correctness of the theory would be to go through the same experiments with the ears effectively stopped.

Since I made these first observations I have noted many other occasions on which minute sound changes have given rise to a correct idea of relationship. Anyone may readily prove for himself in walking in the dark or with the eyes shut along a corridor with doors opening off it, some of which are open and others closed, how easy it is to recognise when one comes opposite one of the open doors, and a very little consideration will convince him that the explanation lies in the difference in resonance from the walls of the corridor and from the space into which the open door leads. Again, I have more than once noticed, when riding on top of a tram-car in the crowded city, that I have been "sensible" of another passenger sitting quietly down on the seat behind me, not through any sound that he has made, but by his cutting off from my ears a portion of the general roar of traffic. It is the finer sound indications of this type, to which we customarily pay little heed, since our eyes yield us more rapid and more complete information, that convey so much information to the blind, whose ears, if not more keen, are more intent, and the blind man's stick undoubtedly serves, not only to feel his way with, but by its tap to supply a source of sound the resonance of which may be noted. There is still much haziness, even among those who have to do with the management of the blind, as to their psychology, and one superintendent of a blind asylum with whom I am acquainted, indulging in that mysticism which at the present day is so fond of explaining phenomena, of which by experiment one may learn something, by theories of which we know nothing, would drag in that blessed word "telepathy" to explain the blind man's knowledge of surrounding objects.

(2) The second principle involved, viz. the reflection of the heat of the face from adjacent surfaces, is not so easily verifiable. I feel fairly confident, however, that

accurate observations with a delicate surface thermometer would show that the cheek was receiving a certain amount of reflected heat as it was approached near to a solid object. That the skin of the cheek is peculiarly sensitive to the degree of temperature will be readily admitted by anyone who has seen a laundress testing the proper heat of her iron by holding it to her face. Further, the repetition of the experiment with the use of a mask, which would minimise the sensitiveness of the skin to changes of temperature, has struck me as likely to give conclusive results, and I am particularly interested to find this supposition supported by Dr. McKendrick's statement that the blind do not so readily avoid an obstacle if the face is covered.

CHARLES H. MELLAND.

Manchester, May 29.

The Pollination of the Primrose.

IN NATURE of May 20 the reviewer, in the course of his appreciative and interesting notice of my book, "Life-histories of Familiar Plants," states:—"We notice that, without stating definitely what insect pollinates the primrose, the author refers to the bee or moth as doing it, in a misleading way. He would have been wiser to ask readers to notice what insect is really effective in the case of this plant. Neither honey-bees nor moths are known to be so." Regarding this point, on p. 78 I have written as follows:—"Now, watch the occasional bee that makes a visit to these two different types of flowers. Here is one alighting. With the sudden weight thus imposed upon it the flower sways," &c. This passage, of course, refers to a humble-bee, as the reference to "the sudden weight" clearly implies. It is true that I did not definitely state that it was a humble-bee, but, on the other hand, I have nowhere in the chapter referred to the honey-bee.

Probably the reviewer, and also readers of NATURE, will be interested in the two following notes from my diary for this year:—April 21: "Saw the first small white butterfly of the season, in garden, about 2 p.m. It was sipping nectar from a primrose flower." (Amongst the photographs illustrating the book referred to above it will be remembered that there is one showing a green-veined white butterfly feeding amongst primrose flowers.) May 3: "A species of large, black humble-bee in garden visiting only primroses and polyanthus. Saw five of them within the space of two yards. One was a large female (the largest humble-bee that I have ever seen), and was apparently entirely black. In some of them, the pollen baskets stood out distinctly as yellow patches on their legs. One other specimen had an orange-coloured thorax." I could not at the time make a capture of one of the bees, and as cold weather followed, and the primroses had nearly done blooming, I did not see the bees again.

While possessing very little knowledge of the species of humble-bees, I am inclined to think that the species I saw was *Bombus harrisellus*, the large specimen being a queen, the one with the orange-coloured thorax a male, and the remainder neuters. Perhaps some of your readers can give some information regarding these bees, and may have observed them on primroses. So far as my observations went, the bees confined their attention exclusively to the primrose family.

JOHN J. WARD.

Rusinurbe House, Somerset Road, Coventry, May 25.

REFERRING to a question raised in NATURE of May 20 (p. 345), the writer of the article "Recent Studies on Animal and Plant Life" may accept it as a fact that the primrose flowers are visited both by humble-bees and by moths, among which may be particularly named the humming-bird and bee hawk-moths. The flowers are also frequented by dipterous insects, a specimen of one of which is enclosed, by which, for the long-styled form at least, pollination may perhaps be sometimes effected.

W. E. HART.

Kilderry, Londonderry, Ireland, May 24.

THE determination of the insects that pollinate the primrose is an old problem, and my remarks in the review under consideration were made with the view of eliciting